# CHOLERA.

## BY THE SAME AUTHOR.

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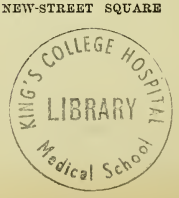

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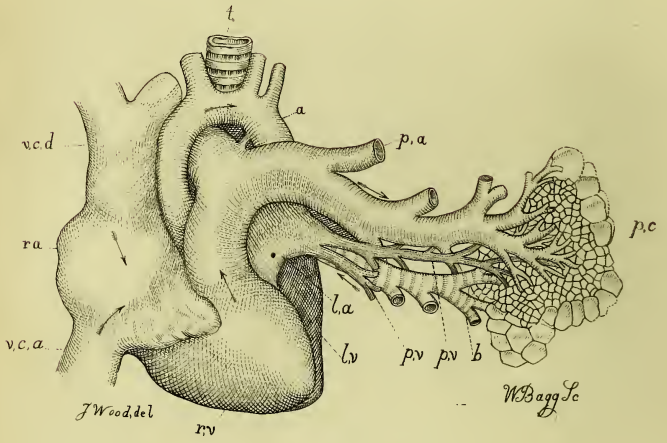


A Diagrammatic representation of the State of the Heart, Lungs and Large Vessels when death has occurred during the Stage of Collapse.

# NOTES ON CHOLERA

## ITS NATURE AND ITS TREATMENT.

BY

GEORGE JOHNSON, M.D. Lond.

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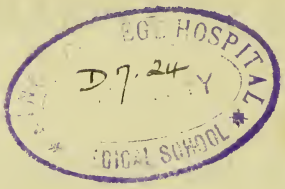


'Homo naturæ minister et interpres, tantum facit et intelligit, quantum de naturæ ordine, re vel mente observaverit; nec amplius scit, aut potest.'

BACON'S *Novum Organum.*

'There is no well-attested case on record of any theory having been abandoned because it produced dangerous results. As long as a theory is believed, men will ascribe its evil consequences to any cause except the right one. And a theory which is once established will always be believed until there is some change in knowledge which shakes its foundation. Every practical change may, by careful analysis, be shown to depend, in the first instance, on some change of speculative opinions.'

BUCKLE'S *History of Civilization in England,* vol. ii. p. 545, note.

## EXPLANATION OF THE PLATE.

The venæ cavæ, the right cavities of the heart, and the pulmonary artery with its branches, are seen to be distended; while the pulmonary capillaries, the pulmonary veins, the left cavities of the heart, and the aorta with its branches, are comparatively empty. See section v. p. 41 *et seq.*

*v c d* Vena cava descendens.
*v c a* Vena cava ascendens.
*r a* Right auricle.
*r v* Right ventricle.
*p a* Pulmonary artery.
*p c* Pulmonary capillaries.
*p v p v* Pulmonary veins.
*l a* Left auricle.
*l v* Left ventricle.
*a* Aorta.
*tr* Trachea.
*b* Bronchus.

I am indebted to my friend and colleague, Mr. John Wood, for the original drawing, which has been engraved with rare skill by Mr. W. Bagg.

TO

# THOMAS WATSON, M.D., F.R.S.

PRESIDENT OF THE ROYAL COLLEGE OF PHYSICIANS.

Dear Dr. Watson,

To you, without having previously asked your permission, I dedicate this small treatise. Whether you will agree with me in my conclusions, or whether you will dissent, I know not; but of this I feel confident, that having known me for many years, and having been mainly instrumental in assigning to me the position which I now occupy in the Medical School of King's College, you will give me credit for a sincere and earnest desire to learn and to teach the truth.

I am, dear Dr. Watson,

Yours faithfully and obliged,

GEORGE JOHNSON.

# PREFACE.

The main object of this publication is to sweep away an erroneous theory with regard to the nature of cholera; the theory in question being one which, of necessity, carries with it a mischievous mode of practice and an entirely wrong estimate of the influence of treatment on the disease.

The attempt to reconcile the acknowledged facts of cholera with the theory that the worst symptoms of the disease are due to the loss of liquid by vomiting and purging, is as hopeless as was the vain endeavour to explain the movements of our planetary system on the hypothesis that the earth and not the sun is the centre of that system.

The sun *appears* to move round the earth,

and for ages it was believed to do so. So the drain of liquid from the blood *appears*, on a superficial view, to be the cause of choleraic collapse, and that view has obtained almost universal acceptance. Yet this modern theory in pathology is as far from the scientific truth as was that ancient theory in astronomy.

More than ten years ago I had become convinced that the theory in question is inconsistent with the facts of the disease, and after a laborious investigation I arrived at what I believe to be the true interpretation of the chief symptoms of cholera. I embodied the results of my labours in a volume which was published in the year 1855; but the epidemic had then passed away, and my book attracted very little notice.

I have, on various occasions, discussed the chief points of my pathological theory with some of the most distinguished physiologists, pathologists and chemists. In

most instances I have found that my doctrines have received a ready and cordial assent, while *no serious objection has ever been raised to any one of my main propositions.* In the hope of exciting a more general interest in this important subject, I have recently published a series of short papers in the *British Medical Journal.* These papers are here reprinted, with a few changes and additions.

Now that we are threatened with another outbreak of cholera, I feel that the time has come when *it must either be proved that I am wrong or acknowledged that I am right in this matter.* I maintain that the question as to the nature of cholera—the relation of the symptoms to each other and to their primary cause—is of all the great pathological problems that have ever come before us, the one which admits of the most complete and intelligible solution. The relation, too, between theory and practice is here most inti-

mate: so that what has been well and truly said of all our medical studies, is in an especial manner and degree applicable to this question, namely, that 'The subjects with which we have to deal are not matters of mere speculative curiosity, or intellectual amusement, to be taken up to-day, and dismissed, perhaps, with unconcern to morrow, but they involve questions of life and death.'[1]

Many men, doubtless, will find it hard to give up a theory which they have held to be indisputable, and upon which they have long and confidently acted; nevertheless, if truth and the interests of humanity demand this sacrifice, it must be made, and made promptly too.

The circumstances which tend to prevent the ready reception of a new doctrine have been so well expressed by one of the great living masters of our profession, that I

[1] Dr. Watson's Introductory Lecture, Principles and Practice of Physic.

venture to borrow his language on this occasion. Dr. Stokes says,[1] 'There is nothing more difficult than for a man who has been educated in a particular doctrine to free himself from it even though he has found it to be wrong. There is something in the human mind which renders the reception of a doctrine, if it be a bad one, a most dangerous circumstance. It is like the imbibition of a particular poison or miasma. We find that some men who have been once exposed to the miasmata which induce intermitting fever, will for the whole course of their lives, be incapable of getting rid of that poison which has been once received. And thus it is, not only with the material, but with the immaterial world. Thus it is, not only with physical, but with moral or intellectual impressions. There is nothing more difficult than to unlearn. This fostering of false doctrine in the mind is one cause; but there

[1] Clinical Lectures on Fever. Med. Times and Gaz., 1854.

are other causes too. The indolence of many men will prevent them taking the trouble to unlearn. The pride of many men has the same effect; and above all things, there is this, that a very large number of students not only of surgery, but of medicine, although they were taught the technicalities of the profession, the alphabet of the profession, as it were, were not taught what is infinitely more important, namely, how to teach themselves. Now this is the principle, and ought to be the grand object of every teacher of medicine. I believe that no man can be fully and entirely taught anything. He must teach himself, and what the teacher has to do, and what I have ever set before myself, as my highest duty, is to endeavour to teach you how to teach yourselves.'

For the convenience of those who may desire to verify my references, or who, for any reason, may wish to study the literature

of cholera, I append a list of authors to whose labours I am indebted for much of the information which I have embodied in my own writings, and without whose aid I could have made little progress in working out this subject.

11 Savile Row, W.
*December*, 1865.

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AND ON SUBJECTS IMMEDIATELY CONNECTED THEREWITH.

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# CONTENTS.

# NOTES ON CHOLERA.

## SECTION I.

### THE RELATION BETWEEN THE SYMPTOMS OF COLLAPSE AND THE LOSS OF LIQUID BY VOMITING AND PURGING.

THERE are few diseases, the treatment of which has been more influenced by pathological theories than that of cholera. The theory which has gained almost universal acceptance is, that the worst symptoms of the disease are due to the drain of fluid from the blood. The practice which has been based upon this theory is to check the purging, by opiates and astringents, and to restore to the blood its lost constituents by saline injections into the veins. What if this theory be erroneous, and the practice suggested by it injurious? That the theory and the practice are both wrong, I am per-

suaded; and I purpose now to give the reasons for my belief.

First, then, let us inquire, *What is the relation between the symptoms of choleraic collapse and the loss of fluid by vomiting and purging?* It will, I suppose, be conceded by all who are prepared to argue this question, that if the symptoms of collapse are occasioned by a drain of fluid from the blood, there must, as a rule, be a direct relation between the degree of collapse and the amount of liquid which escapes from the blood. Now, so far is this from being the case, that there are few writers of any note or authority upon the subject of cholera who do not either assert distinctly, or record facts from which the inference plainly follows, that not only is there no direct relation between the loss of liquid by vomiting and purging and the degree of collapse; but that these conditions often bear an inverse ratio to each other. In confirmation of this statement, I will now quote some of the best known authors on the subject of cholera.

Dr. Edmund Parkes says: 'My cases bear out the observations of Scot, Jameson, Orton, Kennedy, Copland, and, in fact, almost all the English writers of reputation, that there is absolutely no ratio between these two classes of symptoms' (*i.e.*, between the purging and vomiting and the symptoms of collapse) 'or that they appear even to observe an inverse ratio to each other. Thus, at a period of the case when the algide symptoms were most fully developed—viz., in the last five hours—the purging ceased; in the cases where the algide symptoms were prominent throughout, and which cases were consequently the most malignant and the most rapidly fatal, the passage of fluid from the intestines was oftentimes trivial in degree and shortened in the period of its occurrence. In cases in which the vomiting and purging were excessive, the algide symptoms often came on slowly, and were less marked and deadly.'[1]

[1] Researches into the Pathology and Treatment of the Asiatic or Algide Cholera.

Dr. Parkes, then, in illustration of these remarks, cites some cases in which the frequency of the vomiting and purging was quite out of proportion to the severity of the other symptoms. He afterwards states that 'it may be objected to observations of this kind, that the number of stools is, after all, no certain indication of the amount of fluid passed. This objection would be of weight in cases where the stools were not very different in number; but in some of these cases cited above, we have two or four stools attended by more rapid death than twenty-five or twenty stools; and yet, in the first case, it would be impossible not to suppose the quantity of the fluid passed to be much below that of the second case.' He adds, 'It may be confidently asserted that there is no one who has seen much of cholera who does not know that, exclusive of the mildest forms of the disease, a case with little vomiting and purging is more malignant and more rapidly fatal than one in which these are prominent symptoms.'

With reference to the varieties in the general symptoms of cholera, Scot makes the following statement: 'A frequent variety, the worst of all, is that which is noted for the very slight commotion in the system; in which there is no vomiting, hardly any purging—perhaps only one or two loose stools—no perceptible spasm, no pain of any kind; a mortal coldness with arrest of the circulation comes on from the beginning, and the patient dies without a struggle. This has frequently manifested itself as the prevailing type, and almost all die who are attacked by it.' [1]

The testimony of Bell is to the same effect. He says: 'It has been found that the more violent the prominent symptoms are, the more likely is a cure to be effected; and that when the disease is attended with rapid collapse, little or no vomiting and purging, and no spasm, the prognosis is very unfavourable.' [2]

[1] Report on the Epidemic Cholera.

[2] Treatise on Cholera, Asphyxia, or Epidemic Cholera.

Orton, in several passages of his essay, alludes to the fact that, in the worst forms of the disease, vomiting and purging are slight or quickly cease, or even do not occur at all.[1]

Twining gives cases which illustrate the same general principle. Thus, he reports[2] the case of a gentleman who died nine hours after the commencement of urgent symptoms, in whom there were occasional slight efforts to vomit at intervals of half an hour, and he had only four stools from the commencement to the termination of the attack. And, in another part of his work,[3] he says, 'We often see patients vomiting violently for hours, and others purged profusely for several days without cholera (*i.e.*, collapse) coming on.'

The authors above cited have all observed the disease in India; but the testimony of those who have witnessed it in Europe is in strict accordance with that of the Indian au-

[1] An Essay on the Epidemic Cholera of India.

[2] Clinical Illustrations of the More Important Diseases of Bengal, p. 10.

[3] *Ibid*, p. 37.

thorities. Thus, Magendie, describing cholera as he saw it in Paris in 1832, makes the following statement: 'Some patients have no evacuations, insomuch that one is obliged to excite them; for the evacuations, though associated with the disease, are not one of the most serious symptoms; and those patients who have had copious evacuations have been more easily treated than those who have had none. This is a remark which has been made by many physicians.'[1]

Mr. French states, among other reasons for believing that the intestinal discharges are salutary, that while 'cholera, in its most intense form, produces death instantly without discharges, all those who recover from its attack experience the peculiar discharges more or less.' And, again, in favour of the same view, he says, is 'the ultimate recovery of persons who have continued in a state of collapse for a considerable length of time, often extending to a period of three days, and who,

[1] Leçons sur le Cholera Morbus.

in all instances, sustained enormous discharges.'[1]

Dr. Gull,[2] cites the evidence of several practitioners to the effect that, in many cases, the 'evacuations appeared to be wholly insufficient to account for the fatal collapse.' And one case which came under Dr. Gull's observation, affords a striking illustration of the same principle. 'On a *post mortem* examination, the large intestines contained healthy fæces; whilst in the upper two-thirds of the small intestine, the mucous membrane presented the ordinary changes induced by the cholera process, and the rice-water effusion was abundant.' Dr. Gull adduces this case to show that '*cholera sicca*, in a strict sense, does not occur; for although the disease may be fatal without any evacuation, the intestines after death, in such cases, have been found to contain the rice-water fluid.' It can scarcely be doubted, however, that

[1] The Nature of Cholera Investigated.

[2] Report on the Morbid Anatomy and Pathology of Cholera, published by the Royal College of Physicians in 1853.

when, as in this case, the purging has been insufficient to remove the fæculent contents of the large intestine, the loss of fluid must have been out of all proportion less than in most cases in which recovery takes place. And Dr. Gull, in another part of his report,[1] admits that 'the intensity of the symptoms is often in no inconsiderable degree greater than can be accounted for by the amount of the effusion.'

With respect, then, to the question whether there is any direct relation between the loss of fluid by purging and the symptoms of collapse, we have a large amount of concurrent testimony to the effect that no such relationship exists.

Even admitting that there were a direct and constant relationship between the loss of fluid and the degree of collapse, further evidence would still be required to prove that they stand to each other in the relation of cause and effect. It might be that they are

[1] Report on the Morbid Anatomy and Pathology of Cholera, p. 211, published by the Royal College of Physicians in 1853.

only the effects of one common cause, which while, on the one hand, it gives rise to collapse, on the other excites vomiting and purging. How far this may be the case, we shall have to consider hereafter. In the meantime, it must, I think, be conceded, that the evidence of there being an inverse rather than a direct ratio between the degree of collapse and the loss of liquid by vomiting and purging, is fatal to the hypothesis so generally received and acted upon, that choleraic collapse is caused by the drain of liquid from the blood.

## SECTION II.

ARE THE SYMPTOMS OF COLLAPSE SUCH AS AN EXCESSIVE DRAIN OF LIQUID FROM THE BLOOD WOULD BE LIKELY TO GIVE RISE TO?

With reference to the hypothesis that the characteristic symptoms of choleraic collapse are caused by the loss of the watery constituents of the blood, we have next to inquire, *whether the symptoms of collapse are such as an excessive drain of fluid from the blood would be likely to produce.*

Now, what is the condition of a patient who has suffered a profuse drain from the blood, whether of water alone or all the blood-constituents? What is the effect of a copious hæmorrhage, of a profuse and long continued leucorrhœa, of prolonged lactation, of excessive purging, whether the result of disease or of medicine? Is not the condition of a patient who has been ex-

hausted by such means, of the nature of syncope? There are a small and frequent pulse, a pallid skin, dimness of sight, and *tinnitus aurium*: these symptoms being much increased by the erect posture; and, in extreme cases, the head cannot be raised from the pillow, even for a moment, without the occurrence of syncope. For a patient in this condition to walk or stand, or even to sit up, is simply impossible. Now, there is something in the collapse of cholera which is essentially different from the mere exhaustion which leads to syncope. In fact, almost the only symptom which is common to the two conditions is the extreme smallness and feebleness of the pulse.

One great distinction consists in the remarkable blueness, coldness, and other symptoms indicating that during the collapse of cholera the aëration of the blood is greatly interfered with; while no such symptoms of obstructed respiration occur in ordinary cases of exhaustion from excessive

purging. Another great and obvious distinction is this: that whereas a patient exhausted by a drain of fluid from the blood, and therefore verging on syncope, is unable to assume the erect posture without losing at once his pulse and his consciousness, a patient in the collapse of cholera, whose skin is blue and icy cold and whose pulse is imperceptible or extremely small and feeble, is often able to stand up without becoming faint, and even to walk a distance which must require a considerable amount of muscular exertion.

This is a fact alluded to by several authors; and no one can have watched cases of the disease without having observed the surprising amount of muscular exertion of which even a cold and pulseless patient is capable. It is scarcely necessary to quote authorities for this statement; but I will refer to one in illustration of what has been advanced.

Scot says:[1] 'Instances are not wanting of

[1] Report on the Epidemic Cholera, p. 24.

patients being able to walk, and to perform many of their usual avocations, even after the circulation has been so much arrested that the pulse has not been discernible at the wrist.' The same author states, in another place, when speaking of the effects of blood-letting:[1] 'It is remarkable that, in a disease like cholera, syncope should be so rare a symptom.' And, again,[2] 'During the progress of this disorder, when the nervous energy seems to be almost annihilated, and the functions of the heart and arteries to be abolished, this symptom (syncope) is yet very rarely observed.'

Another remarkable difference between the collapse of cholera and the exhaustion caused by an excessive drain of liquid from the blood, consists in *the rapidity with which a patient often recovers from the former condition.* As an instance of this I may quote the following:[3] 'I have seen' (says

[1] Report on the Epidemic Cholera, p. 58.
[2] *Ibid*, p. 28.
[3] Dr. Gull's Report, p. 135.

Mr. Grainger) 'a man stand at his door on Wednesday, who on Monday was in perfect collapse.' And this observation, as Dr. Gull observes, is in accordance with the experience of others.

With reference to this remarkable feature of cholera, Twining makes the following statement:[1] 'In cases not fatal, the progress of recovery is often almost as rapid as the accession of cholera; and if the disease be treated at the very onset, it is not uncommon to see a person well on the third day, after an attack of the worst symptoms, which had commenced with coldness and collapse, and who, if left without remedies, would probably have died in six or eight hours. *In these instances recovery seems almost as sudden and complete as in cases of patients who are resuscitated after suspension of animation from submersion in water.*' It is scarcely necessary to insist upon the fact, that no such instances of rapid recovery

[1] Clinical Illustrations of the more Important Diseases of Bengal, p. 20.

from extreme prostration consequent on a drain of fluids from the blood are ever known to occur; nor, from the nature of things, is it possible that a great loss of blood-constituents can be restored with such extreme rapidity.

The natural and obvious inference from these facts appear to be, that there is an essential difference between the condition of a patient who has been exhausted by a profuse drain from the blood, and that of one in collapse with cholera. It is, therefore, incumbent on those who maintain that choleraic collapse is due to the loss of fluid by the intestinal canal, to explain, if they can, the remarkable differences which have here been pointed out between the symptoms of collapse and those of ordinary syncope.

## SECTION III.

DOES THE EFFECT OF VARIOUS AND OPPOSITE MODES OF TREATMENT ON THE SYMPTOMS OF COLLAPSE AFFORD ANY SUPPORT TO THE THEORY THAT A DRAIN OF LIQUID FROM THE BLOOD IS THE ESSENTIAL OR THE CHIEF CAUSE OF THAT CONDITION?

We have shown in the two previous sections that two classes of facts stand in bold opposition to the theory that the collapse of cholera is due to a drain of liquid from the blood. We have seen that there is no such direct relation between the degree of collapse and the amount of liquid discharged from the blood as must exist if the hypothesis in question were true. We have also seen that the symptoms of collapse differ essentially from those which an excessive drain of fluid from the blood is known to produce. We have now to inquire *whether the effect of various and opposite*

*modes of treatment upon the symptoms of collapse affords support to the theory that a drain of liquid from the blood is the essential or the chief cause of that condition.*

*The Effect of Alcoholic Stimulants.* The condition of a patient in collapse—cold and pulseless, and apparently exhausted—is one which naturally suggests the use of stimulants. Any one who has witnessed the speedy improvement in the pulse and in the other symptoms which usually follows the administration of wine or brandy to a patient who is fainting from loss of blood or exhausted by excessive purging, might reasonably expect to obtain similar results from the same means in the collapse of cholera. Accordingly, stimulants have been given, and given freely and boldly; and the result has been a very general conviction that in the stage of collapse they are not only useless, but positively injurious. Again and again have I seen a patient grow colder, and his pulse diminish in volume and power, after a dose of brandy, and apparently as a direct re-

sult of the brandy. Dr. Gull[1] states that, 'although opium and diffusible stimuli—brandy, camphor, and ammonia—were useful at an early stage of the disease, as collapse set in, they not only failed to produce any favourable result, but often aggravated the symptoms.'

Dr. Paine, who has given an admirable description of cholera and its treatment in 1832, writes thus of stimulants:[2] 'We have seen no benefit from their liberal use, and it is even doubtful whether they contribute much in any quantities. It requires the conviction of experience, however, to enable us to abstain from their use, and to resist the impulse to apply them to the dying spark.'

The very general conviction as to the worse than uselessness of alcoholic stimulants in the collapse of cholera is the more

[1] Report on the Morbid Anatomy and Pathology of Cholera, p. 185.

[2] Letters on Cholera Asphyxia as it has appeared in New York, p. 42.

to be relied upon, inasmuch as it has been forced upon men's minds in opposition to preconceived notions and prevailing theories. The action of stimulants in the collapse of cholera being obviously very different from their influence upon patients who have been exhausted by the loss of blood-constituents, we infer that syncope and choleraic collapse are pathological conditions essentially different; and this conclusion is confirmed in a most striking manner by the effect of other modes of treatment.

*The Effect of Venesection on the Symptoms of Collapse.* It is scarcely necessary to assert that no sane practitioner would think of abstracting blood from a patient who has been reduced to a state bordering on syncope by any of the common sources of exhaustion which have before been referred to (p. 11). It is obvious that the loss of blood in such cases might be attended with perilous and even fatal results. But what has been the effect of venesection in not one or two, but in a large number of cases of cholera, and in the

hands of many different practitioners? Has the effect of this treatment been such as to afford support to the theory that collapse results from loss of liquid? or has it been to add to the cumulative evidence which stands opposed to that theory? We will endeavour now to answer these questions.

It is in the writings of the Indian practitioners that the largest amount of evidence is to be obtained as to the influence of blood-letting in cholera. Scot makes the following remarks on this subject[1]: 'The abstraction of blood, unless as an antispasmodic, is a remedy so little indicated by the usual symptoms of cholera, that its employment in the cure of this fatal disease has afforded a signal triumph to the medical art. It requires no common effort of reasoning or reflection to arrive at the conclusion that, when the powers of life appear to be depressed to the lowest degree, the pulsation of the heart all but extinct, the natural heat of the body gone, and the functions of the system sus-

[1] Report on Epidemic Cholera, p. lviii.

pended, and incapable of being revived by the strongest stimulants, the abstraction of blood might yet prove a remedy against a train of symptoms so desperate. Few remedies,' he says, 'on a fair trial, have been more generally and unequivocally advocated than free blood-letting; and the most that has been urged against it is, that it is not always successful.' He then quotes reports from medical officers furnishing very striking testimony to the benefit of bleeding in cases of extreme collapse.

Annesley states (quoted by Scot, p. lx): 'In place of syncope being produced by bleeding, in the cases which I have treated, the pulse has invariably improved, and the feelings of faintness and debility disappeared.'

Bell makes the following statement[1]: 'The effect of blood-letting would indeed sometimes appear almost miraculous. A patient will be brought in in a cot, unable

[1] Treatise on Cholera Asphyxia, p. 118.

to move a limb, and, but that he can speak and breathe, having the character, both to touch and sight, of a corpse, yet will he, by free venesection alone, be rendered, in the course of half an hour, able to walk home with his friends.'

Rogers gives the following description of the effects of venesection by a medical man who was himself the patient[1]: 'There was a sensation which I am at a loss to describe, as if my heart was ceasing to beat, and a dread of suffocation; this sensation was instantly relieved by bleeding, and I recovered immediately.'

The following striking case is recorded by Sir Ranald Martin[2]: 'On visiting my hospital in the morning, the European farrier-major was reported to be dying of cholera. I found that during the night he had been drained of all the fluid portion of his

[1] Reports on Asiatic Cholera in Regiments of the Madras Army, by Samuel Rogers, p. 259.

[2] The Influence of Tropical Climates on European Constitutions, 6th ed., p. 349.

blood. His appearance was surprisingly altered; his respiration was oppressed; the countenance sunk and livid; the circulation flagging in the extremities. I opened a vein in each arm, but it was long ere I could obtain anything but trickling of dark treacly matter. At length the blood flowed; and by degrees its darkness was exchanged for more of the hue of nature. The farrier was not of robust health; but I bled him largely when he, whom but a moment before I thought a dying man, stood up and exclaimed, "Sir, you have made a new man of me." He is still alive and well.'

Now, let me ask, is it possible to reconcile facts of this kind with the theory that the collapse of cholera results from a loss of the liquid constituents of the blood? If Sir R. Martin's hypothetical statement, that his patient 'had been drained of all the fluid portion of his blood,' were an accurate expression of facts, can we conceive it possible that he could have 'made a new man' of him by abstracting largely the blood which

remained in the vessels? I maintain that the numerous well authenticated instances of great and immediate and permanent relief by means of venesection in the collapse stage of cholera, are utterly and hopelessly irreconcilable with the hypothesis in question.

*The Effect of Purgatives.* If the symptoms of collapse were due to the drain of liquid from the blood, and its escape by the intestinal canal, it would seem to be impossible that the symptoms of collapse should pass away while the drain of liquid by vomiting and purging is continually going on. It would seem, too, that the action of purgatives during the state of collapse must greatly increase the mortality. I do not here propose to consider the merits of the purgative plan of treatment. I wish only to refer to the indisputable fact that there are on record numerous well authenticated instances of recovery from collapse, while the intestinal discharges were encouraged by repeated doses of emetic and purgative medicine. And, further, I

challenge the advocates of the theory which I am endeavouring to refute, to refer to a single case of recovery from collapse in which the intestinal discharges have not continued, in a greater or less degree, while the symptoms of collapse were passing off. If the theory in question had a basis of truth, the cessation of the intestinal discharges must always, and of necessity, precede recovery from collapse.

During the early part of the cholera epidemic of 1849, all the cases of cholera admitted into King's College Hospital were treated by liberal doses of brandy and opium. Under this mode of treatment, the mortality was very great. The treatment was then entirely changed; brandy and opium were discontinued; and large quantities of salt and water were administered. The effect of this treatment was to excite frequent vomiting, and certainly not to check, but rather to increase, the purging; and the result was a much larger proportion of recoveries than under the previous mode of

treatment. I had no share in conducting the treatment on that occasion; but I was greatly struck by the different effects of the two opposite modes of treatment. I was also deeply impressed by observing that, during that epidemic, the arrest of the purging by opiates was in several instances *followed* by the worst symptoms of collapse; and a painful question arose in my mind, whether the collapse in such cases was not a direct result of the arrest of the purging.

At the commencement of the last epidemic—that of 1854—I had arrived at the conclusion that the commonly received theory of choleraic collapse is erroneous. I had the chief charge of the hospital during the whole period of the epidemic; and I gave emetics and purgatives to all the patients who came under my care. I have since published full particulars of all my cases.[1] I am convinced that in many

[1] On Epidemic Diarrhœa and Cholera: their Pathology and Treatment. With a Record of Cases. Longmans and Co.

instances I gave an excessive quantity of castor-oil; yet the result was a mortality, to say the least, below the average mortality in cases of equal severity. During that epidemic, many cases of choleraic diarrhœa came under my observation—cases in which there were vomiting, bilious purging, and cramps. These were all treated by castor-oil, without opiates. They all recovered; and not one case so treated passed into collapse. Several of the medical officers, pupils, and nurses, and a considerable number of patients who were in the hospital for other diseases, had the premonitory symptoms of cholera. All were treated in the same way, and all recovered. In contrast with this most satisfactory result, stands the fact that, during the previous epidemic of 1849, several inmates of the hospital, nurses and patients, having been seized with choleraic symptoms, and being promptly treated by opiates, passed into a state of collapse and died.

I see no way in which the facts here

stated can be reconciled with the hypothesis that the worst symptoms of cholera result from the loss of liquid, and that the main object of treatment is to check the vomiting and purging.

*The Effect of Injecting Hot Saline Solutions into the Veins.* It is well known that the injection of a hot saline fluid into the veins during the collapse of cholera has often been followed by great temporary relief. The pulse improves; the temperature rises; the countenance becomes natural; the voice recovers its strength; and, in short, all the worst symptoms speedily disappear—usually, however, to return with all their former severity within a very short time. The late Dr. Mackintosh of Edinburgh, during the summer of 1832, injected the veins of 156 patients, of whom only twenty-five recovered. There are probably but few practitioners who now expect any practical benefit from this mode of treatment—few who would consider it right to repeat this experiment. But there are many pathologists who main-

tain that, since all the symptoms of collapse speedily disappear after the injection of a certain quantity of liquid into the veins, this experiment proves conclusively that the symptoms which previously existed must have resulted from the loss of a liquid similar in character to that which the operation restores to the blood. I believe, however, that the true explanation of the manner in which the hot saline injections afford the surprising temporary relief which they are acknowledged to have done, has been missed; and that rightly interpreted, the results of this experiment afford as little support to the hypothesis that collapse depends on loss of fluid, as do the effects of other modes of treatment to which reference has already been made.

In a future section I will give what I believe to be the true pathology of collapse; and then I hope to explain the *modus operandi* of the saline injection into the veins.

Before attempting to give what I believe

to be the true interpretation of the symptoms of cholera, I have thought it desirable to direct attention to some of the facts and arguments which are opposed to the commonly received theory. There is so much of the superficial appearance of truth in the theory which explains the symptoms of collapse by the loss of the watery portion of the blood, that the practice of giving opium and other astringents to arrest the intestinal discharges will continue more or less, in spite of failure and disappointment, until it can be clearly shown that the state of collapse has an entirely different origin and cause from that which the theory in question assumes.

## SECTION IV.

A GENERAL VIEW OF THE PATHOLOGY OF CHOLERA. SOME POINTS OF ANALOGY BETWEEN CHOLERA AND SMALL POX.

In previous sections, I have adduced some facts and arguments adverse to the commonly received theory that the worst symptoms of cholera are mainly due to the drain of water from the blood by vomiting and purging. If this theory be erroneous, it is of incalculable importance that it should be discarded; for it is unquestionable that it has had immense influence on the treatment of cholera, as well as on the estimate which men have formed of the effect of various modes of treatment. Let a practitioner be thoroughly persuaded that the essential cause of choleraic collapse is a drain of liquid from the blood, and it will be difficult to convince him that opium and astringents

can do harm, or that an emetic or a purgative can do anything but harm in the treatment of that disease.

A recent writer in the *Lancet*, Oct. 21st, p. 461, says: 'Were we a cholera patient, we should pray to be delivered from men who have only one idea.' Yet it is manifest that this writer is of the class from which he would pray to be delivered. His one idea is, that the secretions are suppressed 'for want of serum; and that by supplying the materials of this, and by the use of means that shall act astringently, we are taking the best means to restore secretions.' What does it avail to refer one, whose mind is thus prepossessed by a theory, to such facts as are contained in the very interesting paper by Mr. Watkins, published in a recent number of the *British Medical Journal*, Oct. 28th, p. 445? Mr. Watkins there states that, in 1854, the deaths under various modes of treatment, but mostly with opium, having been more numerous than the recoveries, at a period, too, when 'the epidemic was in-

creasing both in the number of cases and in severity,' he treated twenty-one cases by repeated doses of castor oil; and, of the cases thus treated, nineteen recovered. About the same period his colleague, Dr. Lett,[1] treated seven cases by full doses of opium, and 'every case had died.' What will be said of facts like these by a man whose one idea of the treatment of cholera is, that loss of fluid is to be counteracted by astringents? He will probably argue, that those who recovered while taking purgatives did so in spite of an erroneous and mischievous treatment; while those who died under the opiate treatment succumbed to the disease in spite of a treatment which was theoretically correct, and which ought, therefore, to have saved them.

I am convinced, from a careful study of the history and literature of cholera, that there will be no general agreement as to the treatment of the disease until we have freed

[1] The case of Dr. Lett himself is one of peculiar and most painful interest.

ourselves from erroneous theories as to its pathology and the relation of its symptoms to each other. With this conviction, I purpose now to set forth, as clearly yet as briefly as I am able, certain facts and conclusions regarding the pathology of cholera which appear to me to be well established.

That the symptoms of cholera result from a *morbid poison*, which may enter the blood either through the lungs or through the gastro-intestinal canal, is a point so generally admitted, that I will not now stop to discuss the question, or to adduce any of the numerous facts upon which this view is based.

The most constant and characteristic effect of this poison is to excite a copious secretion from the mucous membrane of the stomach and bowels. This secretion is tinged with bile *before* collapse comes on, and again *after* collapse has passed off; while during the stage of collapse it has the characteristic rice-water appearance, and bile can be detected only by chemical tests.

The vomiting and purging constitute the means by which the morbid secretions are eliminated from the alimentary canal.

It is certain that the cholera stools contain some poisonous materials by which the disease may be communicated. One fact confirmatory of this view is the frequency with which washerwomen and their families have suffered from coming in contact with the soiled linen of cholera patients. It is probable therefore, that the poison is eliminated through the gastro-intestinal canal. It is also probable that the secretion from the mucous membrane of the digestive canal, together with the vomiting and purging, are as much parts of the natural process of cure as is the eruption on the skin in the case of small-pox. This, at any rate, is certain, that, as no patient ever yet recovered from small-pox without the appearance of the characteristic eruption, so no sufferer from cholera was ever known to get well without more or less of vomiting and diarrhœa.

As, in the worst and most malignant forms of small-pox, the patient may die of blood-poisoning before the rash appears, so, in the worst and most malignant cases of cholera, he may die of collapse without either vomiting or purging, or with little of either. There is no direct relation, as we have seen[1] between the degree of collapse and the amount of vomiting and purging—often rather an inverse ratio between them.

There is yet one more point of analogy between small-pox and cholera. As the variolous eruption, which is unquestionably curative, may yet by its very abundance be fatal through its destructive influence on the skin, so the choleraic secretions may be so copious as to kill by exhaustion. Death by exhaustion is certainly not a common result of cholera; yet, with the known possibility of such a result, a rational eliminative treatment, while it endeavours to free the stomach and bowels from the morbid

[1] Section i, p. 1 *et seq.*

secretions which have been spontaneously poured into them, makes no direct attempt to increase the amount of excretion from the blood.

It has been suggested by some writers, that the analogy of small-pox and its treatment affords an argument against the eliminative treatment of cholera. It is said, with truth, that when the treatment of small-pox was conducted on the theoretical principle of assisting the development of the pustules and so the elimination of the poison, by keeping the patient in a close and heated atmosphere, the mortality of the disease was much increased. It would have been strange indeed if the mortality had not been increased by such a mode of treatment; but it is a result of shallow observation and reasoning, to infer that there is any analogy between that mode of treating small-pox and the use of emetics and purgatives in cholera. The unhappy sufferer from small-pox, who was covered with heaps of bed-clothes and confined in an

artificially-heated and poisonous atmosphere, with closed doors and windows, was not only deprived of the refreshing influence of cool and pure air, but he was compelled to inhale again through his lungs the morbid poison which had escaped from his skin. This treatment was the exact opposite of eliminative. To eliminate is to put *è limine*, or out of the door; but this irrational treatment rendered elimination impossible, by closing the doors and windows through which the poison might have escaped.

So far from there being any analogy between this destructive mode of treating small-pox and the treatment of cholera by emetics and purgatives, the analogy would be much closer with an opposite mode of treatment—one by which a cholera-patient should be made to swallow his own evacuations, or one which approaches very near to this in its results; namely, that which has for its object to restrain the evacuations by opium and astringents, thus increasing the risk of the morbid secretions being reabsorbed into the blood.

There is no known cure for cholera—there probably never will be; but, as the cooling treatment of small-pox which, in spite of violent opposition, was first introduced by Sydenham, materially lessened the mortality from that terrible disease, so the general adoption of an eliminative treatment of cholera—by means of emetics, mild purgatives, and copious draughts of water—would, I am persuaded, do much to lessen the mortality from this great modern scourge.

The analogy of small-pox and the results of the two opposite modes of treating that disease, are certainly in favour of, and not opposed to, this view, as some writers have too hastily assumed.

## SECTION V.

### THE SYMPTOMS AND PATHOLOGY OF CHOLERAIC COLLAPSE.

THE symptoms of choleraic collapse are so well known as to need no minute description. The most important and characteristic of them are the following: coldness and blueness of the skin; great diminution of the volume and force of the pulse; shrinking of the features, with a corpse-like sinking of the eye-balls; more or less hurry and difficulty of breathing, with a short, dry cough; a peculiar feebleness of the voice; coldness of the tongue and breath; a sensation of burning heat in the epigastric region; great thirst; more or less complete suppression of bile and urine; vomiting and purging of a rice-water fluid; torpor and drowsiness in a variable degree, but without delirium; and, lastly, cramps in the muscles. Most of these symptoms are pre-

sent in every case of collapse; some, however, may be absent.

What is the pathological explanation of this remarkable train of symptoms? The one great central fact is this, that, *during the state of collapse, the passage of blood through the lungs from the right to the left side of the heart is, in a greater or less degree, impeded.* Let us now consider the evidence of there being this impediment to the pulmonary circulation; and let us endeavour to ascertain the probable cause and consequences of this obstruction to the flow of blood.

Very conclusive evidence as to the existence of impeded pulmonary circulation during life is afforded by the appearances observed in the heart, blood-vessels, and lungs after death. The *post mortem* appearances within the chest have been described, with more or less minuteness, by several authors; but by no one, I believe, with so much care and accuracy as by Dr. Parkes.[1]

[1] Researches on the Pathology and Treatment of the Asiatic or Algide Cholera.

In the great majority of cases in which death has occurred during the stage of collapse, the right side of the heart and the pulmonary arteries are filled, and sometimes distended, with blood; while the left cavities of the heart are generally empty, or contain only a small quantity of blood; the auricle being partially and the ventricle completely and firmly contracted. The tissue of the lungs is, in most cases, of pale colour, dense in texture, and contains less than the usual amount of blood and air. With respect to the extremely anæmic condition of the lung, when death has occurred during the stage of collapse, there is an entire agreement amongst those authors who have most accurately described the *post mortem* appearances. There is something surprising, as Briquet and Mignot observe,[1] in the contrast between the almost constant occurrence of this extremely anæmic condition of the lung, from which scarcely even a few drops of

[1] Traité Critique et Analytique du Cholera Morbus.

blood flow when the tissue is cut, and the hyperæmia of most of the other viscera.

There is a remarkable contrast, too, between this anæmia of the lung when death has occurred during collapse, and the great engorgement of the lungs which is almost invariably found when death occurs in the febrile stage which often follows reaction.

Now, it is evident, from the appearances here described that, during the stage of collapse, there is an arrest of blood in the branches of the pulmonary artery *before* it has reached the pulmonary capillaries (see frontispiece). The arrest at this point explains the remarkable anæmia of the texture of the lungs, during collapse, while the hyperæmia of the lungs after reaction is due to engorgement of the pulmonary capillaries. Before I attempt to explain this remarkable arrest of blood, it may be well to allude briefly to certain phenomena in the living but collapsed patient which afford confirmatory evidence that the pulmonary circulation is greatly impeded.

The impeded flow of blood through the lungs resulting, as it must, in a very scanty supply of blood to the arteries, accounts for the character of the pulse in cholera. It accounts, too, for the fact that the pulse has often been observed to increase in power and volume under the influence of venesection, which, by relieving the over-distension of the right cavities of the heart, increases their contractile power.[1]

Another appearance which receives explanation from the small stream of blood in the arteries, is that of the shrinking of the integuments, and especially the collapse of the features and the sinking of the eye-balls. The eyes of a patient in deep collapse are often as much sunk as those of a corpse; and the chief cause of this, in the case of both the cholera-patient and the corpse, is the more or less complete emptiness of the branches of the ophthalmic artery.

[1] See Dr. Reid's essay, On the Effects of Venesection in Renewing and Increasing the Heart's Action under Certain Circumstances.

That the arterial stream during collapse is reduced to a minimum, is proved by the fact that arteries of considerable size have been opened during life without the escape of blood. Magendie states[1] that, on one occasion, he cut across the temporal artery of a patient in collapse and no blood escaped. ('Il ne s'écoula pas une goutte de liquide') Scot says[2] that the temporal artery having been frequently opened 'little or no blood could be obtained, the artery merely emptying itself in a languid stream, not in a jet, and then collapsing.' He also states that a surgeon, despairing of other means, cut down upon the *brachial* artery; but so completely had the circulation failed, that no blood flowed.'

While, therefore, the small and feeble pulse, the collapse of the features, and the occasional absence of hæmorrhage from a wounded artery, are explicable on the supposition that the arteries receive a very

[1] Leçons sur le Cholera Morbus, p. 21.

[2] Op. cit., p. xxx.

scanty supply of blood, these phenomena afford evidence confirmatory of that derived from *post mortem* appearances, that during the stage of collapse the passage of blood through the lungs is much impeded.

It appears, then, that, during the stage of collapse, the blood which is sent into the pulmonary artery is, in great part, arrested in the minute branches of the artery *before* it reaches the capillaries of the lungs. What is the cause of this arrest of blood? Some writers have suggested that the blood has been rendered so thick by the loss of serum, that it cannot pass through the minute vessels. This theory is scarcely deserving of a moment's consideration. It is entirely at variance with the fact before referred to (Sec. I.), that there is no direct relation between collapse and loss of fluid by the bowels; and again, with the fact that the state of collapse passes off, while loss of fluid by purging continues, and while, therefore, the thickening of the blood which, according to this theory, has stopped its passage through the lungs, should be con-

tinually increasing (Sec. III.). The suddenness with which collapse often occurs, is quite inexplicable by the theory that thickening of the blood through loss of its water is the cause of that condition. In illustration of the sudden occurrence of collapse, I may refer to the following passages in Sir William Burnet's *Report on Cholera in the Black Sea Fleet in* 1854. 'The first to be attacked were men already on the list for diarrhœa, several of whom fell into a state of collapse one after the other; but, about the same time, robust healthy men, who had fallen suddenly down in a state of collapse, began to be brought in from various parts of the ship (the *Britannia*), even from the yards, where they were seized while reefing sails.' And the surgeon on board the *Albion* reported that 'the attacks in many instances were so sudden, that many men fell as if they had drunk the concentrated poison of the upas-tree.' The supporters of this theory of blood-thickening as the cause of collapse would have us believe that, in the course of a few minutes, the blood of these robust

men had become so thickened by the loss of water as to be incapable of transmission through the minute vessels of the lungs. I shall hereafter show that the thickening of the blood is a *consequence* and not a *cause* of the arrest of blood in the pulmonary arteries.

I believe the true explanation of the arrest of blood in the lungs to be this. *The blood contains a poison whose irritant action upon the muscular tissue is shown by the painful cramps which it occasions; the blood thus poisoned excites contraction of the muscular walls of the minute pulmonary arteries, the effect of which is to diminish, and in fatal cases entirely to arrest, the flow of blood through the lungs.*

We have seen that the condition of the lungs after death during collapse affords conclusive evidence that the arrest of the blood occurs, not in the capillaries, but in the branches of the pulmonary artery, before the capillaries are reached by the blood. We know that the walls of the arteries are



muscular, and that they have the power of contracting upon their contents under the influence of a stimulus, such as cold, electricity, or mechanical irritation. I suppose that no physiologist at the present day would deny that spasm of the arteries is as real a fact as spasm of the muscles.[1]

Many experiments and observations prove that contraction of the muscular walls of the arteries has great influence on the passage of their contents. For instance, it is a well known fact, that the tissues of an animal immediately after death cannot, without a force which endangers the integrity of the vessels, be injected with any of the coloured fluids which are commonly used for that purpose. The coats of the arteries, so long as their vital tonicity remains, contract upon their strange

[1] The modern and most approved theory relating to the loss of consciousness which occurs at the commencement of an *epileptic seizure*, is that it is due to 'arrest of the cerebral circulation, owing to contraction of the vessels, through irritation propagated along the vaso-motor nerves from the medulla oblongata.' See Dr. Russell Reynolds *on Epilepsy*, p. 244.

contents, and impede the passage of the injection into the capillaries.

Nearly a century ago, Hales[1] performed some ingenious experiments to demonstrate the power which arteries possess to control the flow of various liquids through them. His experiments were performed on animals recently killed; and he found that, while warm water passed very readily through the arteries, cold water, decoction of bark, and brandy, passed much more slowly.

Some experiments performed by Blake[2] on living animals bear upon this question. He found that a concentrated solution of a salt of soda, when injected into the jugular vein of a dog, killed the animal in less than a minute. On examination after death, the right side of the heart was found greatly distended, while the left contained only a little black blood. A few grains of nitrate of silver in solution destroyed life

[1] Statical Essays, 1769.

[2] Edinburgh Medical and Surgical Journal, vols. liii., liv. and lvi.

in precisely the same way. The passage of blood through the lungs is arrested, and the animal dies with the right cavities of the heart distended, while the left cavities are nearly empty.

The sudden arrest of the flow of blood through the lungs in consequence of the accidental admission of air through a wounded vein in the neck or axilla has some relation to this subject. The entrance of air has usually been indicated by a peculiar gurgling sound in the wound, and the symptoms which rapidly follow are thus described :[1] 'Speedy occurrence of syncope,[2] which is either preceded by a cry, with the expression, "I die," "I am dead," "I suffocate," or by anxiety and tremblings. Or, without any such precursors, the syncope rapidly reaches such a degree that all consciousness is lost, and the patient falls down; cold sweat breaks out on the forehead; and, in

[1] Dr. John Reid's Physiological, Anatomical, and Pathological Researches, p. 553.

[2] Strictly speaking this is not syncope but a form of asphyxia.

a quarter of an hour, sometimes sooner, sometimes later, he is dead.' After death, both in the human subject, and in animals that have been the subjects of experiments, the right side of the heart is found to be much distended with frothy blood ; and the same mixture of air and blood is usually found in the pulmonary arteries. The left side of the heart is usually empty ; but a small quantity of frothy blood is occasionally found in the left cavities and in the aorta. The cause of death in these cases is the distension of the right side of the heart, which results from the impeded transit of frothy blood through the vessels of the lungs. Mr. Erichsen found, by experiments on a dog recently killed, that beaten bullock's blood, mixed with air, required nearly twice the pressure to drive it through the pulmonary vessels, that would suffice to drive unmixed blood through the lungs.[1]

I have referred to these experiments in illustration of the general principle : 1, that

[1] Edinburgh Medical and Surgical Journal, vol. lxi.

the movement of blood through the lungs may be quickly arrested by the addition of some foreign ingredient to the blood; and 2, that this arrest is probably due to the power which the arteries possess to contract upon their contents.

We can now understand the sudden coming on of collapse, and its sudden passing off. Robust men falling down 'as if they had drunk the concentrated poison of the Upas tree,' and recovering again almost as rapidly as 'patients who are resuscitated after suspension of animation from submersion in water' (see p. 15).

The proof that the blood is arrested during the stage of collapse in cholera is, as before stated, partly the anatomical condition of the lungs after death, partly the symptoms observed during life.

About two years since, I had under my care in the hospital a woman (S. B.) who was suffering from dropsy, the result of heart disease of long standing. On going through the ward one afternoon, I saw her sitting

up in bed as well as usual. In less than five minutes after I left her to go into another ward, I was sent for in consequence of her having become suddenly worse. I found her gasping for breath, cold, and pulseless, with a blue and shrunken appearance of the features, exactly resembling the collapse of cholera. I at first thought that she would die in a few minutes; but she rallied in some degree, and lived forty-eight hours, during which time her dropsical legs became rapidly gangrenous. We found, as I had predicted, that, besides old standing disease of the mitral valve, there were firm fibrinous coagula in the branches of the pulmonary artery. The sudden obstruction of the pulmonary arteries by fibrine caused a state of collapse exactly resembling that of cholera.[1]

The most interesting and conclusive evidence that arrest of blood in the lungs is the true key to the pathology of choleraic

[1] Hospital Case-Book, vol. xix, p. 59.

collapse, is to be found in the simple yet complete explanation which it affords of all the most striking chemical phenomena of the disease—the imperfect aëration of the blood, the fall of temperature, the dark and thick appearance of the blood, and the suppression of bile and urine.

*Chemical Consequences of the Obstructed Flow of Blood through the Pulmonary Arteries.* It is obvious that the stream of blood from the pulmonary capillaries to the left side of the heart is the channel by which the supply of oxygen is introduced into the system. One necessary consequence, then, of a great diminution in the volume of blood transmitted to the left side of the heart, must be that the supply of oxygen is lessened in a corresponding degree. This position probably will not be disputed by any one who will give the subject a moment's consideration. Nor, again, can it be denied or doubted that certain obvious results must of necessity follow this limited supply of oxygen.

The combustion of those constituents of the blood which are normally subjected to the action of oxygen will be diminished in proportion to the deficiency of that gas; and thence follows simultaneously and of necessity a reduction of temperature, and so scanty a formation of urinary and biliary constituents, that, while the state of collapse continues, the functions of the kidneys and liver are virtually suspended. The blood at the same time has that black, thick, treacly appearance which is not peculiar to the collapse of cholera, but which is common to it with all diseases which are attended with a defective aëration of the blood. The blood has this character during the cold stage of a severe ague fit. Dr. Mackintosh and others, who have bled patients during the cold stage of ague, describe the blood as flowing from the arm at first in a slowly trickling stream, being of a dark colour, and not coagulable.[1] Dr. Dundas

[1] See Mackintosh's Practice of Physic, vol. i, p. 86 *et seq.*

Thomson, who published an elaborate paper on the Chemistry of the Blood in cholera,[1] states that one specimen of blood from 'a patient who laboured under an affection of the mucous membrane of the air-tubes,' contained a greater excess of solids, in proportion to water, than he had found even in cholera-blood.

The blood in cholera is black and thick only during the stage of collapse; in other words, during the stage of pulmonary obstruction and defective aëration. This state of the blood bears no relation to the loss of water; it comes on when the loss of water has been very trifling; it passes off rapidly, while loss of water by purging continues unchecked. It is simply a result of defective aëration, just as the thick and smoky flame of a lamp is a result of defective aëration.

The chief constituents of bile and urine and carbonic acid are all results of oxidation; none of them can be formed without

[1] Med.-Chir. Trans., vol. xxii, p. 67.

a large supply of oxygen. Suppression of bile and urine during the stage of collapse is a necessary consequence of the limited supply of oxygen which results from the obstruction in the lungs. The amount of carbonic acid expired during collapse is also much diminished.[1]

One fact confirmatory of the view that the very scanty formation of bile, urine, and carbonic acid during collapse is a result of the diminished supply of oxygen, is, that *the secretion of milk continues apparently undiminished.* I have myself observed this fact, and it has been mentioned by several authors.[2] Thus Magendie states that, one of his patients having been delivered of a child a few days before she was seized with cholera, the secretion of milk continued so

[1] See on this point particularly, Twining's Clinical Illustrations, etc., 2nd ed., p. 15.

[2] See on this point Magendie, Leçons sur le Cholera Morbus, 1832, p. 27; Dr. Hutchinson, History and Observations on Asiatic Cholera in Brooklyn, New York, in 1854, p. 10; Dr. Robertson, Edinburgh Monthly Journal, 1848, p. 393; and Dr. Gairdner, 'On the Pathological Anatomy of Cholera,' Edinburgh Monthly Journal, July, 1849.

abundant through a first and a second collapse, which ended fatally, that it was necessary to empty the breasts, in order to relieve the pain which their distension occasioned. Now, if the other secretions are suppressed on account of the deficiency of water in the blood, why does the secretion of milk continue? Magendie says, because the blood reaches the breasts, and supplies the materials for their secretion, on account of those glands being nearer to the heart than the liver and kidneys, which, being at a greater distance, do not receive the supply of blood necessary for the discharge of their functions! My explanation of this remarkable and instructive fact is simply this. The chief constituents of milk—casein, sugar, oil, and water—may be obtained from the blood without the addition of oxygen. The secretion of milk, therefore, continues during the stage of collapse; while the highly oxygenised secretions are suspended, their essential constituents being formed only in very minute quantities during

that stage, on account of the defective supply of oxygen.

*Explanation of the great temporary Relief resulting from the Injection of a Hot Saline Solution into the Veins.* No theory of collapse can be considered complete which does not give a satisfactory explanation of the great temporary benefit which immediately follows the injection of a hot saline solution into the veins. I have before referred to this subject (p. 29), and I deferred my explanation until I had given some account of the phenomena of collapse.

I have adduced many facts and arguments in proof of the position that the essential cause of collapse is an arrest of blood in the lungs, occasioned by a spasmodic contraction of the muscular walls of the pulmonary arteries. If this be the actual cause of collapse, we might, *à priori*, expect that for a time it would be removed by the injection of a hot fluid into the veins. The temperature of the fluid injected by Dr. Mackintosh varied from 106° to 120°; but

he states that 'the good effects of the injection were rapid in proportion to the heat of the solution.'[1] The hot fluid, rapidly mixing with the blood in the right side of the heart and in the pulmonary artery, would, as it were, dilute the poisoned blood and render it less irritating, just as diluents render the urine less irritating to an inflamed bladder or urethra. It is probable, however, that the chief action of the injection would be to relax the spasm of the minute arteries by its high temperature.[2]

[1] Op. cit., vol. i, p. 365.

[2] Dr. Parkes, who tried the injection in some cases, appears to have obtained much less striking results from its employment than Dr. Mackintosh and others describe. (Op. cit., p. 219.) The probable reason of this is, that the temperature of his injection was too low. In one case, he says, it did not exceed 98°; while in another it was 'tepid.' In the other cases, the temperature is not stated. I think, therefore, that Dr. Parkes's failure to do much good by a tepid injection is confirmatory of my view, that the high temperature of the injection employed by Dr. Mackintosh and others had more influence than the mere mixture of water with the blood. I believe that the hottest room of the Turkish bath would quickly relax the spasm of the pulmonary arteries; and so, *if it did not cause faintness*, it might be a real help to recovery.

Thus, the impediment to the circulation being overcome, the blood rapidly flows on to the left side of the heart and the arteries, and the phenomena of collapse pass away with marvellous rapidity. The benefit, however, is of but short duration; for the primary cause of the impeded circulation—namely, the poisoned condition of the blood—being still in operation, and the originally hot solution being cooled down by its diffusion through the entire mass of the circulating blood, the stream of blood through the lungs will soon again be obstructed; and the patient thus passes into a state of collapse as profound as before and yet more hopeless. It appears, therefore, that the hot saline injection into the veins, and the operation of venesection, when it rapidly relieves, as it often has done, the symptoms of collapse, have this effect in common, that they facilitate the passage of the blood through the lungs, and thus lessen that embarrassment of the pulmonary circulation which is the essential

cause of choleraic collapse. But, whereas the hot injections act by removing the impediment which results from spasmodic contraction of the arteries, venesection acts by relieving over-distension of the right cavities of the heart, and thus increasing the contractile power of their walls.

I now ask those pathologists who have hitherto thought that the temporary benefit following injections into the veins affords conclusive evidence that collapse results from loss of water, to consider whether the explanation here offered is not at least as probable, as complete, and as consistent with all the known facts of the problem, as that which is commonly received.

## SECTION VI.

STATEMENT OF FACTS IN CONFIRMATION OF THE HYPOTHESIS THAT A MORBID POISON IN THE BLOOD IS THE ESSENTIAL CAUSE OF CHOLERA.

I HAVE before stated, p. 35, that the presence of a morbid poison in the blood, as the essential cause of cholera, is very generally admitted. There does, however, exist some scepticism with regard to this point; and therefore, it seems not undesirable to state briefly the chief facts and arguments which are in favour of the doctrine in question.

Now, the proofs of the existence of morbid poisons are to be found mainly in the history of the diseases to which they give rise, and in the impossibility of giving a rational interpretation of the morbid phenomena without assuming the existence of some morbific agent. Neither chemistry nor the microscope has thrown any light

on the essential nature of morbid poisons. 'It is the human body,' as Dr. Carpenter well observes[1] 'which forms the appropriate testing-apparatus for morbid poisons; and even if we could always obtain them in a separate state, and could subject them to chemical analysis, we should know much less of their most important properties than we can ascertain by observation of their actions in the system; this alone affording the means of judging of their *dynamical* character, which is of far more importance than a knowledge of their chemical composition.'

The hypothesis, that a poison in the blood is the cause of the gastro-intestinal symptoms of cholera, is supported by many analogous facts. For instance, vomiting and diarrhœa not unfrequently result from breathing air which is contaminated by the exhalations from decomposing animal matter. Thus Dr. Christison states[2] that M. Ollivier, 'while

[1] Human Physiology.

[2] Treatise on Poisons, 4th ed., p. 636.

visiting a cellar where old bones were stored, was seized with giddiness, nausea, tendency to vomit, and general uneasiness; and subsequently he suffered from violent colic, with profuse diarrhœa, which put on the dysenteric character, and lasted for three days.'

Breathing the air of the dissecting-room and the dead-house is a common cause of diarrhœa; and Mr. Simon[1] remarks upon the instructive fact that, when a diarrhœa has been excited by dissecting animals whose flesh has a peculiar odour, this odour may sometimes be detected in the evacuations of the patient. Some years since, I was present while a medical friend examined the dead body of a patient who had died of cancer of the bladder and bowel. The stench was singularly strong. Within a few hours, my friend was seized with diarrhœa; and he noticed that his stools had the strong and peculiar odour which had pervaded the room when the body was being examined. In such cases as this we have

[1] Lectures on General Pathology, p. 231.

conclusive evidence that fœtid gases may pass, with the inspired air, through the lungs into the blood, and thence be eliminated through the bowels. Can it be beneficial to prevent or retard that process of elimination?

It has been proved by experiments on animals, that many of the symptoms of cholera may result from the injection of putrid matters into the blood. Mr. Henry Lee, in his *Pathological and Surgical Observations*, describes several experiments of the kind, which were followed by vomiting, diarrhœa, difficult breathing, great prostration, and death.

The remarkable arrest of blood in the branches of the pulmonary artery, which I have shown to be the essential cause of choleraic collapse, has its exact analogy in the arrest which is caused by the injection of certain salts into the blood, or by the admission of atmospheric air into the veins.[1]

Evidence of blood-poisoning in cholera

[1] See *ante*, pp. 51 and 52.

may be derived from a consideration of the *symptoms of invasion*, as they have been called. The pathological theories which have unhappily prevailed in this country have led to an undue regard being paid to diarrhœa as a premonitory symptom of cholera, and to an almost entire disregard of other symptoms, to which the Indian practitioners attach great importance. The symptoms in question are those indicative of general discomfort and derangement of function, particularly affecting the nervous system. Twining says[1]: 'Prior to the more distinct and alarming attack, there are sometimes for a few hours, and in some cases for two or three days, symptoms of indisposition, evident not only to the patient himself, but to his friends. When cholera is raging severely, the disease is often ushered in by diarrhœa; at other times it begins with catarrh, nausea, and oppression at the scrobiculus cordis, which are not in an early stage to be distinguished from the slight in-

[1] Op. cit. p. 9.

disposition which often precedes fever. The approach of cholera in this manner makes the patient suppose he is feverish or bilious; and *if recourse be had to some of the medicines commonly used in slight ailments of that sort, the disease is said to be caused by the dose of medicine taken, when, in fact, it had been insidiously making progress for some hours.*'

In this country, it has not unfrequently been asserted that an attack of cholera has been caused by the operation of a rhubarb pill or a dose of castor-oil. I can give a parallel to this *post hoc ergo propter hoc* argument. Some years since, when I was seeing out-patients at the hospital, a woman brought a child who was suffering from febrile symptoms, and for whom I prescribed a mixture containing nitrate of potash. Two days afterwards, the mother returned, and said, in a complaining tone, 'Your medicine has brought out the small-pox.' Truly the small-pox had come out; but certainly it was in the blood before it came into the skin. So when symptoms of

cholera follow speedily on the action of an aperient, the morbid poison was before in the blood, and probably caused the feeling of derangement for which the dose was taken. It is as probable that small-pox might result from rubbing croton-oil on the skin, as that a specific disease like cholera would be caused by a purgative.

Annesley gives an extract from the letter of 'a zealous and intelligent medical officer,' who strongly insists on the great practical importance of studying the initiatory symptoms of cholera. Referring to the purging, vomiting, and spasms, his correspondent says: 'I am so thoroughly convinced that these symptoms are only secondary, that, were the following marks present, I should not hesitate to pronounce the case one of epidemic cholera. As the patient is approached, an appearance of overpowering lassitude is at once perceived, with a pallid, anxious, and sorrowful cast of countenance; and, in more advanced stages, the countenance is dejected and sunk.' He then refers

to a case in which he observed this peculiar expression of countenance; and, feeling confident that cholera was impending, he kept a close watch upon the man. After an interval of nine hours, vomiting, purging, and cramps set in.

Bell and Orton[1] describe the initiatory symptoms of cholera in much the same terms as Annesley; and Dr. Paine, whose experience of the disease was obtained in New York, says: 'Diarrhœa and vomiting do not always distinguish the premonitory stage; but it is sometimes denoted only by headache, loss of appetite, oppression at the chest, etc.; and, again, spasms are known to have been the earliest symptom, and at first the only prominent one.' These symptoms of general derangement, which often attend the invasion of cholera, appear to be strictly analogous to those which mark the commencement of other undoubted zymotic blood-diseases.

Again, Orton and others of the Indian

[1] Op. cit.

authors have remarked on the striking resemblance between the symptoms of cholera and those which result from the bite of a snake or other venomous animal. The venom of these animals unquestionably enters the blood, and thus often destroys life. So, it is probable, does the cholera-poison enter the blood.

Another fact, which is almost certainly indicative of a morbid condition of blood in cholera, is the frequent occurrence of *albuminuria* during the progress of the disease. This symptom, in connexion with cholera, cannot, I think, be a result of a merely passive congestion of the kidney. Much more probably is it due to an active congestion excited by a morbid quality of blood.

One of the most conclusive arguments in favour of the doctrine that a morbid poison in the blood is the essential cause of cholera, is based upon the fact that the worst symptoms of collapse have often been observed to follow immediately upon the arrest of the vomiting and purging by opiates and as-

tringents. The first case which painfully convinced me of the fatal mischief which may result from the treatment of diarrhœa by opium, occurred during the cholera epidemic of 1849. A woman, about forty years of age, was seized with the usual symptoms of choleraic diarrhœa—vomiting, purging, and cramps. She had not a symptom of collapse. The countenance was natural, the skin warm, the pulse good. I gave her five grains of Dover's powder every hour until three doses had been taken. When I saw her again, in about three hours from the time of my first visit, the vomiting, purging, and cramps had ceased; and she was in full collapse, from which she never rallied. This case gave a terrible shock to my belief that collapse is a consequence of loss of fluid, and that it is to be prevented by arresting the vomiting and purging which usually precede and accompany the symptoms of collapse. I saw no way of escape from the painful conviction, that my patient's condition had been made fearfully worse by

my well-intended but mischievous interference. Since that time I have had frequent opportunities of ascertaining from the published reports of cases, and from what I have seen in the practice of others, that the immediate occurrence of profound collapse is a not uncommon result of the sudden arrest of the vomiting and purging by opium. The salutary and curative efforts of Nature—the vomiting and purging—by which the morbid poison is being eliminated, are thus arrested; the poison then accumulates in the blood: the flow of blood through the lungs becomes obstructed; and the state of collapse is established.

MM. Briquet and Mignot have published some instructive particulars of the results obtained by them in the treatment of diarrhœa by opium.[1] Their practice was to prescribe rest, rice-diet, and from fifteen to thirty drops of 'laudanum of Sydenham;' the dose to be repeated in an hour, and to be followed by opiate enemata, if necessary.

[1] Op. cit., p. 514.

In obstinate cases, they gave altogether as much as from eighty to a hundred drops of laudanum. The result of this practice was, that, out of 200 patients who came under treatment at the commencement of the attack ('dès le début des premiers accidents'), no fewer than twenty-six—that is, 13 per cent.—passed into collapse. This, I have good reason to believe, is a far larger proportion of cases of collapse than would ordinarily occur if choleraic diarrhœa were allowed to take its own course and terminate, as it tends to do, in spontaneous recovery.[1] Those who advocate the use of opiates and astringents in the treatment of

[1] Contrast with this opiate treatment of diarrhœa the practice of Dr. Stilton, who treated choleraic diarrhœa by gr. xx doses of calomel. He says, 'This mode of treatment succeeded in overcoming, in 223 individuals, the symptoms which usually precede cholera; though without such a system these persons would probably have passed into a state of collapse. In these 223 cases I found it necessary to repeat the calomel, to one individual five times, to some others three times; but in the generality of cases a single dose was sufficient.' *The Cholera in Malta in* 1837, *from the Italian of Guiseppe Stilton, M.D.*, by Seth B. Watson, M.D., 1848, p. 124.

choleraic diarrhœa appear to forget that there is such a result as spontaneous recovery, and they claim the credit of a cure in every case of diarrhœa so treated which does not pass on to collapse. During the last epidemic of cholera (in 1854), the late Mr. Wakefield stated, in a letter to the *Times*, that in a large number of cases of diarrhœa occurring amongst the prisoners at Cold Bath Fields, the only medicines which he gave were carbonate of soda and mint-tea; and not a single case passed into collapse. It can scarcely be supposed that this plan of treatment had any other effect than to dilute the contents of the bowel, and so to assist their speedy expulsion—a mode of operation entirely different from that of opium and astringents.

Mr. French states, with reference to the treatment of diarrhœa[1]: 'I am satisfied from much experience, that cases of epidemic diarrhœa generally subside speedily under the use of the simplest possible remedies

[1] The Nature of Cholera investigated, 2nd ed., 1854, p. 75.

which are wholly free from astringent properties.' In this statement I entirely concur.

But it may be asked how it could happen, if there be a morbid poison in the blood the escape of which it is dangerous to arrest by opium, that so large a proportion as nearly seven-eighths of MM. Briquet and Mignot's patients escaped without more serious consequences? Ought not the whole number thus treated by opium to have passed into a state of collapse? To these questions it may be replied that, happily for the patients, in a large proportion of cases, the diarrhœa continues, in spite of repeated doses of opium, for a period varying from a few hours to several days; and, in such cases, it is reasonable to suppose, that the curative efforts of Nature succeed in eliminating the morbid poison from the blood, notwithstanding the opposing influence of the drug.

During the last epidemic of cholera, I saw several cases in which a diarrhœa had for some days gone on, in spite of large and repeated doses of opium and astringents. In

each of these cases, the diarrhœa quickly ceased after the exhibition of one or two doses of castor-oil. And it is evident that the experience of MM. Briquet and Mignot was in perfect agreement with this; for they state that if, in spite of the energetic employment of opium, the diarrhœa continues for a period of two days, opium is then of no avail; and they have found in such cases that an emetic of ipecacuanha has immediately put a stop to the disease. It is probable that a diarrhœa which thus continues for several days while opiates are being given, and which is so speedily arrested by an emetic or a purgative, is due, not to the continued presence of a morbid poison in the blood, but rather to the local irritation of the mucous membrane of the digestive canal by the morbid secretion which has been poured into it, and the complete escape of which has been retarded by the opium.

During the last epidemic of cholera a so-called 'Treatment Committee,' appointed by the General Board of Health, endeavoured to

ascertain by statistics the most successful treatment of the disease. This was evidently a hopeless task, for in the vast majority of the returns several remedies having different and even opposite modes of action, were so jumbled together that no clear and definite result as to the effect of any one remedy or plan of treatment could come out of these statistics. One of their tables is intended to show what treatment was most successful in preventing diarrhœa from passing into cholera. The comparison between calomel alone and calomel in combination with opium, is interesting. When calomel alone was given in cases of diarrhœa only 2·4 passed into collapse, but when the calomel was combined with opium no less than 6·9 per cent. passed into the more advanced stage of the disease. In other words, when, in the treatment of diarrhœa the purgative action of calomel is lessened by its combination with opium, the risk of the disease passing into the stage of collapse is nearly three times as great as when calo-

mel is given alone. The table containing these figures will be found at p. 26 of the Report of the Treatment Committee. We have before seen that of the cases of MM. Briquet and Mignot, which were treated by opium alone, about 13 per cent. passed into collapse. These figures all point to the conclusion that to check the diarrhœa by opium is to increase the risk of collapse.

The peculiar character of the intestinal discharges in cholera affords some evidence of blood-poisoning.

Boehm[1] was the first to publish the fact, that the secretions discharged during life, and those which are found in the bowels after death, contain a large amount of epithelium.

The flocculi in the rice-water stools consist almost entirely of perfectly organised epithelial cells, most of them of large size. Of this fact, I have satisfied myself by repeated examinations of the discharges from

[1] Die Kranke Darmschleimhaut in der Asiatischen Cholera, 1838.

different patients. The peculiar creamy viscid secretion, which sometimes nearly fills the small intestines after death, is almost entirely made up of the same fully formed epithelium. Now, it is obvious that this large amount of epithelium cannot be explained by the peeling away of one or two layers of cells from the surface of the mucous membrane—the result of a local irritation during life, or of maceration by the fluid contents of the bowel after death. This abundant cell-formation can result only from a very active vital effort. And if the object of that cell-growth be not to withdraw from the blood some morbid products —some constituents of the blood or of the tissues which have been damaged by the morbid poison—it is difficult to suggest any explanation of the phenomena.

The desquamation of the skin and of the uriniferous tubes of the kidney, under the influence of the poison of scarlatina, is precisely analogous to this intestinal desquamation in cholera. In both cases, it is probable

that a poison in the blood is the immediate cause of the desquamative process.

The very peculiar odour of the rice-water stools—so entirely unlike any other odour, that it alone would suffice for the diagnosis of cholera—shows that the stools contain some new material, or that some constituent of the body has undergone a peculiar and specific morbid change.

It is nearly certain that the intestinal discharges contain a morbid poison by means of which the disease may be communicated to others. So that, as Dr. William Budd has suggested,[1] a single case of cholera may, through the sewers, infect a whole district.

The facts and arguments here adduced are sufficient to warrant the inference that *a morbid poison in the blood is the essential cause of cholera.* The poison excites the gastro-intestinal symptoms, the vomiting and purging; by means of which the poison and its products are eliminated. And, in the worst cases, the same

[1] Association Journal, 1854.

poison excites contraction of the muscular walls of the pulmonary artery, and then the condition of collapse occurs. During an epidemic of cholera, there occur many cases of choleraic diarrhœa which never pass into collapse. It is probable that both classes of cases are of essentially the same character; the same morbid poison is the cause of both; and the transition from choleraic diarrhœa with bilious stools to choleraic collapse with rice-water stools takes place by almost imperceptible gradations in different cases. The transition back again from the state of collapse to that of bilious diarrhœa occurs when the morbid condition of the blood is so far lessened that the circulation through the pulmonary vessels again becomes free. The stage of reaction is then established; and this is sometimes followed by fever, with engorgement of the lungs and a scanty secretion, or even complete suppression, of urine, which is often fatal.

## SECTION VII.

### THE GENERAL PRINCIPLES OF TREATMENT.

THE account which I have now given of the pathology of cholera, although brief, will, I trust, have been sufficient to convey to those who have carefully read these pages a definite idea of the doctrine which I advocate. If there be any who desire to have further details and proofs and illustrations, these may be found in a book which was published ten years ago, but which has hitherto attracted singularly little notice.

I proceed now to the inquiry, What has this pathology to do with treatment? And I maintain that it is a compass which will guide the intelligent practitioner over a 'sea of troubles' and perplexities in the treatment of cholera. We now have a definite view of the nature of cholera, of the relation which the symptoms bear to

each other, and of the manner in which the disease tends to a spontaneous recovery. We can see that, amongst a vast number of plans of treatment, some have been unquestionably beneficial by assisting the curative efforts of nature, while others have been as unquestionably detrimental by opposing those efforts. Others, again, have been of a mixed character; as, for instance, the combination of calomel—a purgative—with opium. And, lastly, some methods have been simply neutral in their effects, except in so far as the knowledge that something was being done for him may have inspired the patient with hope and confidence. As an illustration of this, I may mention one of the many infallible cures which have found their way into the *Times*—I mean the inoculation with quassia. If that which has been satirically said contained the whole truth—namely, that 'the chief art of the physician is to amuse the patient while nature performs the cure'—the inoculation with quassia would be as

effectual as any other method of treatment, and it would have the great negative merit of doing no harm.

There is no remedy which has the slightest pretensions to be considered a cure for cholera; no drug or agent which, so far as we know, will neutralise the poison or lessen its virulence. I have not the faintest hope or expectation that a specific remedy for such a disease as cholera will ever be discovered. The number of really specific remedies which we possess for any diseases is unfortunately very small. Those who believe in specifics are, in general, ignorant of the nature of disease and of the true methods of cure; and this ignorance renders them quite incompetent to estimate the influence of treatment.

If I have rightly interpreted the phenomena of cholera, the natural method of cure is eliminative. The process of elimination takes place by two successive stages. 1. There is excretion from the blood into the stomach and bowels; and 2. The morbid

contents of these viscera are ejected by vomiting and purging.[1] Can we do anything to assist this natural curative process? Unquestionably we can; by emetics and purgatives. But is it necessary or beneficial to interfere in any way? Ought we not rather to leave the cure entirely to nature? I have no doubt that much may be done with advantage. And my conviction of this is based partly upon theoretical grounds, and partly upon the actual results of treatment. If we carefully observe the condition of a patient in collapse, we shall often find that the intestines are more or less distended with fluid; and this, too, while perhaps there is general torpor and little or no effort at expulsion. Again, it is often found that, although there has been copious watery purging during life, the small intestines contain, after death, a large amount of a peculiar viscid dirty white material, having

[1] If there are any who doubt the elimination of a *morbid poison* by the stomach and bowels, no one can doubt the existence of offensive *morbid secretions* which require to be cast out.

a very offensive odour. A purgative may be useful in removing both these conditions; namely, over-distension of the bowel by liquid, and accumulation of offensive viscid semi-solid secretions. The object of a purgative in cholera is not to increase excretion from the blood into the stomach and bowels; but simply to assist in the expulsion of the morbid secretions from the digestive canal. To fulfil this indication, that purgative is best adapted which acts most speedily, yet with the least amount of irritation. I believe that castor-oil best accomplishes this object; but a great variety of purgatives have been given with more or less success.

When, in August 1854, I first publicly stated in the *Medical Times and Gazette* that I was giving castor-oil as a *purgative* in cholera, the announcement was received with a shout of dismay. Give a purgative to a patient whose blood has already been drained of all its liquid and thus rendered too thick to circulate! Fearful

malpractice! Statistics must immediately be published to show that this plan is most deadly in its effects.[1] But at the same time statistics were showing that calomel in frequent and sometimes in large doses is a very successful remedy. How does the calomel act? Is it not an irritant and a violent purgative? Or what is there to deprive it of its purgative properties when given for the cure of cholera?

[1] It was evidently the opinion of some who chanced to be in prominent positions at that time, that my alarming outbreak of heterodoxy must be treated, like cholera, by very active repressive measures. So fierce an onslaught was made upon me, that I began to fear lest what Sydenham said of small-pox two hundred years ago, would be found equally applicable to cholera in the present century. 'I venture to assert,' said Sydenham, 'that the physician who has much to do with small-pox runs many risks with his reputation. The vulgar are ever in the habit of ascribing deaths to the officiousness of the attendant; whilst physicians themselves catch greedily at opportunities for slander. They make out their case before incompetent judges, and procure most uncharitable verdicts. They act thus in order that they may build up a name for themselves, upon the ruined reputation of others; a proceeding disgraceful to even honest artisans—doubly disgraceful to scholars.' The *Works of Sydenham*, translated by Dr. R. G. Latham for the Sydenham Society, vol. 1. p. 137.

Calomel a purgative! exclaims indignant orthodoxy. No! calomel is a sedative or an alterative. It acts upon the liver, and restores the secretion of bile. Those who give this explanation of the action of mercury in cholera, being quite unaware what is the real cause of the suspended secretion of bile during collapse, are therefore equally ignorant of the conditions which are essential for the restoration of that secretion. To call calomel a sedative, an alterative, and a restorer of the biliary secretion does not, like its combination with opium, deprive it of its purgative properties. I therefore claim the reported success of the calomel treatment as evidence in favour of elimination.

*Saline Treatment.* Another plan of treatment which has unquestionably been attended with a large amount of success is that which is known as the saline treatment of Dr. Stevens. The theory upon which this treatment is based—namely, that the essential cause of cholera is a deficiency of the saline constituents of the blood—has been

shown by accurate chemical analysis to be fanciful and erroneous; but this happens to be one of those rare cases in which a theory may be wrong yet the practice based upon it not far from right. I have condensed from Dr. Stevens's work[1] the following account of his theory and his practice.

'The first efforts towards a cure should be directed to assist nature in throwing off the poison from the blood, *per vias naturales*, and, at the same time, supplying the necessary saline stimuli, on which the action of the heart and its vessels depends. In accordance with this view, patients presenting merely the first stage of premonitory symptoms, diarrhœa and vomiting, took, on their admission, a Seidlitz powder; and, if sinking was felt but without bowel complaint, more active purgatives were then employed; or three or four teaspoonfuls of Epsom salts were added to the Seidlitz

[1] Observations on the Nature and the Treatment of Asiatic Cholera.

powder. On the bowels being moved, plenty of thin beef-tea, well seasoned with salt, was given. This simple treatment was so successful, that a great many patients had no further complaint, and were generally dismissed cured in a few days.'

'If cramps, coldness, or sinking of the pulse, were present, the patients were considered as cholera-patients in the second stage. The 'non-purgative' salts were administered every half-hour, or more or less frequently, according to the symptoms, and in the following dose: Muriate of soda ℈j; carbonate of soda ʒss; chlorate of potash gr. vij. When life seemed rapidly ebbing, the collapse stage having been reached, a strong solution of the same salts, at a temperature of 100°, was thrown into the bowel. The saline mixture was administered half-hourly; and, in severe cases, the muriate of soda was increased to a drachm, or even more, as circumstances seemed to require. In some cases, an enema, composed of a large tablespoonful

of common salt in water, at as high a temperature as the patient could bear, was administered every two or three hours.'

This plan of treatment has been attended with a large amount of success in the hands of many different practitioners; but, for a very obvious reason, it has not been generally adopted. Its mode of action is essentially eliminative; and, therefore, it is incompatible with that theory of collapse through loss of water which has taken such firm hold of the European medical mind.

This theory of collapse, which now, we trust, has received its death-blow, started into life when cholera first visited Europe in 1832. Before that time, our countrymen in India could not only *give* purgatives as a part of the regular treatment of cholera, but they might also *call* them purgatives without subjecting themselves to the imputation of culpable rashness and folly. Abundant illustrations of the use of purgatives in the treatment of cholera may be

found in the writings of the following Indian practitioners: Bell, Orton, Scot, Curtis, Twining, Christie, Rogers, Kennedy, Searle, and Corbyn. None of these writers had any very definite pathological theory. They looked upon cholera as a disease chiefly of the nervous system. Most of them called it spasmodic cholera; and they often gave opium to relieve cramps at the same time that they gave very active purgatives to remove the morbid secretion from the bowels. The fact that, in many instances, their treatment was a mixed treatment—a combination of opiates with purgatives—renders their writings the more instructive, since it clearly appears, on a careful study of their cases, that, in proportion as either the opiate or the purgative plan prevailed, was the mortality increased or diminished.

Several of these authors speak highly of the use of castor-oil, on account of its unirritating properties, and the completeness with which it brings away the viscid and putrid secretions from the bowels.

I select the following as an illustration of the kind of evidence which may be found abundantly in the writings of the Indian practitioners. In Scot's *Madras Report*, (page 94), Assistant-Surgeon Boyd reports that the first cases admitted were treated with calomel and tincture of opium, repeated as symptoms indicated; 'the first dose was in many cases rejected, but the second seldom failed to sit on the stomach and allay the inordinate gastric irritability and lessen the purging; but it proved such a powerful sedative in most cases, that the patient soon after swallowing the potion fell into a comatose state, from which he could not be roused by the most powerful stimulants administered freely both internally and externally.' He then changed his practice. He began by venesection; as soon as the patient had recovered from the faintness, he gave ten or fifteen grains of calomel, which he washed down with a draught of camphor mixture. 'In a few cases,' he says, 'where slight irritability of stomach remained after

the venesection, fifteen or twenty drops of laudanum were added to the camphorated draught, but I never exceeded that quantity; and in six or seven hours after the calomel was administered a laxative draught, composed of an infusion of senna and salts, or oil, was given to determine to the bowels and accelerate the operation of the calomel; the compound powder of jalap would have been preferred, but I had none in store. My expectation of success after adopting this line of practice was more than fulfilled, as out of twenty-eight admitted, and treated in this way, I lost only two, and both were rather unfavourable cases on admission, from their not applying until the disease was far advanced and the pulse imperceptible.' This report is dated August 1st, 1818.

My friend Dr. Parkes in his able work on cholera, from which I have gained more information on this subject than from any other single book, estimates the mortality amongst his patients in India at about 56 per cent. Then referring to 'the astonishing

success which attended the practice of several gentlemen in the earlier periods in India,' he concludes that the epidemics which he witnessed were above the ordinary degree of severity. That this may in part explain the difference of mortality is possible, yet I cannot but attach much more importance to the opposite modes of treatment. The main object with Dr. Parkes was to check vomiting and purging by opiates, &c.—whereas the earlier Indian practitioners, almost without exception, gave purgatives, and many of them gave purgatives in large and frequent doses.

The very interesting paper by Mr. Watkins[1] to which I have before referred, p. 33, contains a most instructive illustration of the effect of various modes of treatment. It will be seen that four distinct sets of cases are there described. In one set opium was given alone; these were all fatal.[2] In a second set, the opium was combined with

[1] British Medical Journal, Oct. 28th, p. 445.

[2] The cases treated by opium were under the care of Dr. Lett, the friend and colleague of Mr. Watkins.

calomel or with gray powder; of these more than half died. The purgative action of the mercury somewhat lessened the mortal effects of the opium. In a third set of five, calomel alone was given; and of these one died, while four recovered. In a fourth set of twenty-one, castor-oil was given; and nineteen of these patients recovered. Besides these, one case which had been treated by gray powder and opium passed into collapse, and then, castor-oil being substituted for the other remedies, she ultimately recovered. Lastly, one most interesting case in full collapse was treated by castor-oil until he rallied; then, through some mistake, two pills, probably of calomel and opium, were given; he fell again into collapse, was a second time treated by castor-oil, and ultimately recovered.

It is sometimes argued, that no remedies can possibly be of use during collapse because absorption is suspended. Now, in the first place, Magendie has shown that absorption is not absolutely suspended during

collapse, although it is rendered much less active. He injected camphor into the rectum, and detected its odour in the breath in five minutes, whereas in health, he says, it may be detected in one minute. He performed the same experiment with ether, and with a similar result. He reasonably infers that absorption is slowly carried on during collapse.[1] There is undoubted evidence that opium and alcohol become absorbed, and produce narcotic effects during collapse. But the remedies which are of real use during collapse have no need to be absorbed. Surely a mustard or a salt and water emetic need not be absorbed in order to excite the stomach to expel its contents. And it can scarcely be doubted that calomel and castor-oil, by their stimulant action on the inner surface of the bowel, may excite muscular contraction, and so act as purgatives without being taken up by the blood-vessels.

A writer on cholera during the last epi-

[1] Leçons sur le Cholera Morbus, p. 97.

demic stated that 'brandy, ether, and ammonia, turpentine, and even champagne, were given to rouse the flagging energies of life, and to little purpose; they were generally thrown from the stomach as soon as swallowed, and draughts containing creasote, chloroform, and camphor, shared the same fate. The stomach had, as it were, lost the power of ministering to the wants of the system.'

Now, it may be asked, whether the stomach was not much wiser and more useful than the doctor, and whether it could better 'minister to the wants of the system,' than by the expulsion of its morbid secretions, together with such a horrible mixture of drugs as could not be retained and absorbed without adding to the perilous oppression of the vital powers.

During the last epidemic, I observed very carefully the effect of *brandy* in the few cases in which I ventured, in a tentative manner, to give it to patients who were in collapse. I found that it speedily increased

the restlessness and feeling of oppression; at the same time that the pulse lost power and volume, and the temperature fell. I infer, therefore, that brandy and all alcoholic stimulants during the collapse stage of cholera add to the obstruction of the pulmonary circulation, instead of passing freely through the lungs and then stimulating the left ventricle, as they do in other states of system. I resolved, therefore, never again to give alcoholic stimulants in collapse, no matter how great the *apparent* exhaustion may be.

In a few cases, the *sesquicarbonate of ammonia* was given with apparent benefit; and this remedy appears to deserve a further trial as a stimulant during the stage of collapse.

It is worse than useless to attempt to *feed* a patient during collapse. The secretions of the stomach are utterly deranged, and the power of digestion is suspended. The mildest nourishment administered at this time only adds to the feeling of oppression

and general distress, from which the act of vomiting often gives immediate relief. Upon this point I have satisfied myself by repeated and very careful observation.

The wife of the English chaplain in Paris has recently obtained notoriety by administering *chlorodyne* in fifty or sixty cases of incipient cholera, and, as it is stated, 'has succeeded in arresting the disease in every instance.' What would be the effect of chlorodyne in cases of choleraic diarrhœa? It would relieve the cramps, and, by its narcotic operation, it would in some degree retard the spontaneous recovery of the patient. Nevertheless, if a lady have a determination to step beyond her own proper sphere, and to play at doctoring, chlorodyne would be a much less dangerous drug in her hands than opium and strong astringents.

Dr. Bullar and Dr. Risdon Bennett have recently spoken in high terms of the good effects of *external warmth* to the skin of patients in collapse. Dr. Bullar recommends

the hot mustard bath; and Dr. Bennett advises packing in hot wet blankets. There can be no question that to thoroughly warm a patient in collapse is often a real benefit; the pulse, the temperature and the colour of the skin, and the expression of the features, all improve simultaneously. And we can readily understand this effect of warmth if we have thoroughly comprehended the *modus operandi* of the hot saline injection into the veins. But too much must not be expected from the application of external warmth. The warm bath has often been found to have a very depressing effect. Annesley declares that, in his opinion, it did more harm than good. 'The fatigue arising from going in and coming out of it, and from rubbing and dressing the patient, exhausted him.'[1] Christie found the bath injurious; and Dr. Parkes says: 'I have seen a man walk firmly to the bath, with a pulse of tolerable volume, and a cool but not cold surface,

[1] Diseases of India, p. 156.

and in five or ten minutes have seen the same man carried from the bath, with a pulse almost imperceptible, and a cold clammy skin. I cannot find in my notes a single case in which the warm bath appeared beneficial.'[1]

During the epidemic of 1849, I saw several patients, in King's College Hospital, placed in the hot-air bath ; and the result of my observation was that, while the warmth appeared to be beneficial, by relieving the cramps, and sometimes even improving the pulse, yet, on the whole, the patients appeared to be rather distressed than comforted by the bath. The employment of hot baths in the treatment of cholera obviously requires caution. Faintness is a not uncommon result of remaining too long in a hot bath, even when the bather is in health ; and, in the collapse of cholera a sudden and deadly exhaustion may be thus induced. Then, be it remembered, that choleraic collapse is a form of asphyxia.

[1] Parkes On Cholera, p. 210.

There is an instinctive craving for air. Again and again will the patient toss all covering from his body; and it is likely that surrounding the body with hot water, or hot air, or wet blankets, may cause distress by interfering with cutaneous respiration—*i. e.*, the aëration of the blood through the skin. A very convenient way of keeping up the warmth of the body is by the application of hot bottles, hot dry flannels, or hot sand-bags, to various parts of the surface. Friction with stimulating liniments is also of use in restoring warmth and relieving cramps.

Dr. Murray, an Indian practitioner, obtained very striking results in the improvement of the pulse by *injecting into the rectum* every half-hour, or hour, or two hours, according to the urgency of the case, a pint of a weak solution of common salt and carbonate of soda, at a temperature of 120°.[1] The injections were retained generally from two to four minutes—sufficiently

[1] Rogers' Reports on Asiatic Cholera, p. 241.

long to impart considerable warmth to the blood in the vessels of the large intestine; and to this, without doubt, the good effects were mainly due. It appears to me that hot injections into the rectum may be used with advantage in cases of extreme collapse.

The good effects of heat are unquestionable. But what is to be said of *ice* or *iced water*, either taken into the stomach, or applied externally? During collapse there is usually a sensation of burning in the stomach, and a strong craving for cold liquids. Iced water relieves that sensation of heat, and it has a powerful influence in checking vomiting. Are these sufficient reasons for giving iced water to a patient in collapse? In my opinion, decidedly not. I am sure that vomiting, when not excessive, is beneficial; and I believe that iced water lessens the burning sensation in the stomach by diminishing the vascularity of the mucous membrane, and thus interfering with the excretion of the morbid poison, upon the ejection of which by vomiting depends the

patient's recovery. For these reasons, I believe that the administration of iced water to a patient in collapse is injurious. In most cases, I would allow an unlimited quantity of water of the temperature of the room, but, in extreme collapse, I would persuade the patient to drink *hot liquids*, with a view to add some warmth to the blood.

The application of *ice to the spine* has been recommended by Dr. Chapman. The theory which suggested this practice is a speculative web spun from the projector's brain. There is no evidence to show that this practice has been useful in any case. That some patients subjected to this distressing mode of treatment would recover, is certain; it is equally certain that their chances of recovery would be lessened by the depressing influence of the ice on the nervous system, and its chilling effect on the blood. We know that, if we can thoroughly warm a patient in collapse, we improve his general condition; and this,

whether the warming be effected by the external application of heat, or by hot injections into the veins or into the rectum. We therefore can have no difficulty in perceiving that to thoroughly *chill* a patient in collapse must be a painful and a dangerous proceeding.

*Venesection*, as we have seen,[1] has often afforded great relief during the stage of collapse; and I have before pointed out that the only rational interpretation of its *modus operandi* is that, by lessening the over-distension of the right cavities of the heart, it increases the contractile power of their muscular walls. The symptom which appears especially to call for venesection, and which has most commonly been relieved by it, is rapid breathing, with an oppressive sense of suffocation. When, with these symptoms, there is a cessation of vomiting and purging, which is probably a result of the almost entire arrest of the blood in the lungs, I believe that venesection affords the

[1] *Ante*, p. 20 *et seq.*

only hope of benefit. If during the last epidemic I had known as much of the essential cause of collapse, and of the influence of venesection, as I now do, I believe that I might have saved some patients who, for the want of that knowledge, were lost.

*The hot saline injections into the veins.*—It is an interesting question whether, with our present knowledge of the *modus operandi* of the hot saline injections into the veins, these can ever again be employed with the hope of permanent benefit. The mortality after this mode of treatment has hitherto been frightful. In Dr. Mackintosh's cases there were only 25 recoveries to 131 deaths. These is reason to believe that if all these 156 patients had been left without any treatment, a larger proportion would have recovered. It should be borne in mind, however, that this operation has hitherto been performed by those who have erroneously supposed that the main object was to add liquid to the blood, and to prevent its escape by the bowels. It has consequently happened that in some instances

the temperature of the liquid has been too low to have any decided effect on the pulmonary circulation; while, in most instances, the operation has been preceded and followed by the free administration of brandy and opium; so that it is impossible to distinguish between the certainly bad effects of these narcotics, and the possibly good effects of the saline injections, if they had not been interfered with by other means. One circumstance which makes it doubtful whether the saline injections are a safe remedy is the almost invariable occurrence of very severe rigors, which generally commence within a few minutes after the completion of the operation — sometimes during its performance. These rigors, as Dr. Mackintosh admits, 'afford proofs the most decisive of a pathological change in the system.' Whatever may be the explanation of their occurrence, whether they are due to some injurious effect of the solution upon the structure and vital properties of the blood, or whether, as is possible, they result from the sudden trans-

mission into the arteries of the large amount of morbid blood which had before accumulated in the veins—be the cause of these rigors what it may—the fact of their occurrence would make me hesitate to recommend the hot saline injections as a remedy.

In this communication, I have done little more than sketch the general principles of treatment. The only safe guide in conducting the *details* of treatment is to have a very clear and definite view of the pathological interpretation of symptoms, and of the mode in which remedies may assist the natural process of cure. Without this pathological knowledge, it is scarcely possible to avoid falling into some of the many errors which have affected the treatment of cholera, and which have often led to the neutralisation of the good effects of one remedy by the simultaneous use of other means having an opposite and an injurious influence.

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